

opposite side, was in various experiments from twenty to thirty seconds.¹ With reference to prussic acid, Dr. H. Meyer determined by his experiments that killing by this poison, although a rapid process, is by no means so instantaneous as formerly supposed, generally only after the lapse of one minute and a half, and it is well known that this is one of the most rapid of poisonous articles.² The argument, therefore, against the introduction of sedative poisons, based upon the time of their operation, has no intrinsic force.

J. C.

ART. XXI.—*Medical Errors. Fallacies connected with the application of the Inductive Method of Reasoning to the Science of Medicine.* By A. W. BARCLAY, M.D., Cantab. and Edin., Fellow Roy. Coll. Phys., Physician to St. George's Hospital, &c. &c. 12mo. pp. 123. London: J. Churchill & Sons. 1864.

AN extensive subject is here presented; capable of occupying a much larger volume. Medical errors form *pars magna* of medical literature. Diogenes could hardly need to search longer for a man in Athens, than Stuart Mill among medical writers for a logician. Is it not the principal advance of the last century, that we have been relieved of many errors which, if they did not rule medical practice, at least burdened it greatly? A natural effect of the discovery of those errors, and of the unscientific basis of much prevailing practice, has been an era of medical scepticism, in which we now live. It is, therefore, of the utmost importance that a positive science of medicine shall be re-constituted of the materials within reach of the profession. Induction is undoubtedly essential for this reconstruction. That its principles and methods, at once scientifically correct and practically available, should be understood by all medical students, practitioners, and teachers, is of great consequence.

Dr. Barclay has, in his published lectures, strongly asserted this, and has well illustrated it by examples. It may be regretted, however, that he has not been quite so felicitous in his discussion of the philosophy of the subject. To those who have read H. Spencer, Mill, Baden Powell, and Whewell upon similar topics, much is missed of the clearness with which, even with diverse views, those masters of the logic of science deal with it.

Such a criticism ought not to be made without some citation to justify it. Much of his language is clear and correct, but sometimes words are used in a manner liable to create confusion. Thus (p. 10):—

“While it is quite true that the hypothesis is very often suggested to the mind of the accurate observer by some harmony which arrests his attention, it is nevertheless true that in many cases it is a mere deduction; and in most of those which are classed as the highest inductions, there is a combination of both forms of reasoning, often united with an idea which is not the fruit of any reasoning process whatever, but is simply the bright offspring of genius.”

Without an example of such a “bright idea”—which our author does not give—we are unable to imagine, among familiar instances of discoveries by induction, what part of any of them can be regarded as apart from

¹ Pereira, Elements of Mat. Med., vol. i. p. 154.

² London Medical Times, vol. ix. p. 432.

"any reasoning process whatever." Is it not commonly understood that the genius of Kepler, of Newton, or of Dalton, was a genius *for* reasoning?

Objection is made by Dr. Barclay to Prof. Laycock's stated principle, that medical theories are to be grounded on analogy, all their analogies having reference to one fundamental principle, which is stated to be "the unity of structure and function of organisms in time and space." This Dr. Barclay regards as not rightly called an inductive, but a deductive principle, indeed "an *hypothesis* assumed *a priori*, every argument based on it partaking of its hypothetical character." We hold that this ranks Dr. Laycock's principle at least as much too low as he may, perhaps, have placed it too high as a foundation of medical philosophy. The unity of structure and function of organisms is, as a proposition, itself the result of the largest induction. It is the major premise of every syllogism in physiology, pathology, and therapeutics, as the yet larger one of the *uniformity of nature* is held by Baden Powell¹ to lie at the basis of all science, being deposited there by the aggregate labours of observers in all departments; at once the result and the instrument of all induction.

Disadvantage belongs, it appears to us, to the use of the terms "empirical law," as applied by Dr. Barclay; or, again, "empirical law of limited significance" (p. 54); "difference between the laws of nature and empirical laws" (p. 61); "laws of the highest class, which absolutely govern matter, while those of the lowest class are only observable on special occasions, apply to a few individuals, and are liable to constant interruption," &c. (p. 60). Evidently there is here a twofold employment of the word "law," which *ought*, in science, to mean absolute fact, or invariable sequence—the relation in reason of certain things or events; but which he makes sometimes also signify the *statement* or *knowledge* of facts proved, or even only supposed to be shown, on an incomplete induction—mere approximations or hypotheses, not laws at all. These may vary, or be more or less absolute; laws *really* ascertained cannot, though our knowledge of them on complex subjects may often be very imperfect, and we cannot always at once explain exceptions to recognized laws by their conformity to others not understood.

Some will urge, too, that Dr. Barclay gives to the idea of *causation* too dominant a position in his theory of induction, while at the same time he employs it obscurely. Sparing the metaphysics of such a discussion, we must simply quote a few lines from a page (p. 58) intended to make clear "what is a correct induction."

"Let us endeavour, before we proceed further, to ascertain what is meant by the term 'law.' In few words, it is an expression of the mode in which any given cause operates to produce a certain effect."—"It is not at all of the essence of a law to explain the production of any phenomenon, although it may greatly contribute towards such a result."—"We know nothing of the cause of gravitation, although there is no subject of which the laws are better understood. By the discovery of these laws, a great step was made towards an explanation of the causes which govern the motions of the heavenly bodies. In this we have an instance of the most elementary law with which we are conversant, and yet one which offers not the very least explanation of the cause which produces the effect."

In this we do not deny that our author's meaning may be apprehended, but it must be contended that it is not consistently expressed, nor can such language aid much the ratiocinations of a beginner in inductive philosophy.

¹ Essays on Inductive Philosophy, Unity of Worlds, &c., p. 98.

Dr. Barclay's consideration of the subject of "averages," though not always free from ambiguity of expression, is, mainly, very judicious. Algebraic calculation exhibits startlingly the necessity of a large number of cases to prove anything whatever clinically by mere statistics. When there are, for example, only four circumstances supposable whose presence or absence may determine a more or less favourable tendency in a series of cases, the variations possible, and which must be allowed for, are fifteen. With five such circumstances, the variations amount to thirty-one; with ten, to more than one thousand; with fifteen, to thirty-two thousand, and so on, and yet the following is not an over-statement of the circumstances to be taken into the estimate in all clinical observations.

"First, before the attack, the sex, age, and social position of the individual; his previous state of health, including early constitution, acquired habit, and the effect of the relative amount and purity of food and air; his actual condition, whether suffering from any minor ailment (to say nothing of major complications, which may be excluded), from actual privation or cold, or from any recent excess. Secondly, as regards the seizure itself; its immediate cause, its intensity, the rapidity of its development and progress, and the extent to which the special organ attacked is affected by it. Thirdly, the circumstances external to the patient influencing the progress of the disorder, such as his home, the means at his command, the friends that surround him, ignorant or well-informed, his nurse and his food, including stimulants as well as other nourishment; the skill of his medical attendant, and the judgment with which other subsidiary remedies are employed; if necessary, the influence which the conditions calling for their employment exercise over the disease, no less than the remedies themselves; and, perhaps more than anything else, the discretion with which the amount of stimulants is strictly limited to the exigencies of the particular case. Lastly, the wonderful and inexplicable influence of mind over body, the condition of hope or fear, of quiet confidence or restless anxiety." (p. 36.)

Well, indeed, then, may it be said of clinical facts, as it has been of experimental results in another department, "*non numeranda sed ponderanda.*" The practitioner goes through a frequently unconscious analysis of all these varying circumstances, in his estimate of cases occurring under his eye. His experience is worth more or less, according to his power to do this well or ill. But, when the thread of the facts is truly seized, when a "relation in reason" is found, one case to a gifted medical mind may be worth almost as much as a thousand to the mere routinist, or even to the mere statistician. Thus it is that the aggregate experience of the past in practical medicine has accumulated much that is invaluable and permanent with much, also, that is, or was, provisional only. It is as great an error to discard all of the former as to adopt all of the latter. Dr. Barclay does not appear to us to do quite full justice to therapeutical experience as of itself a basis for legitimate induction. "Empirical practice," says he (p. 35), "appeals to no laws, is guided by no analogies, rests on no principles, but simply asserts that experience teaches the benefit of the plan proposed. On no better grounds rests the ordinary saline treatment of fevers, &c."

We would hold that no better ground can be asked for on behalf of this or any other treatment, when it is shown that experience, carefully weighed, really does teach the benefit of it. That is the great question; its explanation comes afterwards. When that is simple, and obviously included in our knowledge of laws before ascertained, as in the case of chemical antidotes to definite poisons, all is rational as well as experimental. But who does not know that nine-tenths of what is really valuable in medicine was first discovered, often by accident, and, if explained at all, this followed

long since? How much is owing, at last, to what Dr. Barclay calls "deductive reasoning" upon physiological or bare pathological facts? Perhaps enough, and no more, to prevent our adopting altogether the maxim ascribed to Rousseau, that "rationalism in medicine conducts only to absurdities." Dr. Barclay must, however, be credited with a full exposition of the application of direct observation and experiment in therapeutics, in the instances of the cure of ague by bark and its preparations, and of vaccination.

The most valuable as well as the larger part of the volume before us is occupied with examples of fallacious reasoning upon special medical or collateral subjects. Even reports made upon returns given to the British Medical Association appear to be open to animadversion on this ground. To the first of these therapeutical inquiries the name of Prof. Bennett, of Edinburgh, is attached.

"The reporter on acute pneumonia is well known to have expressed very decided opinions on the treatment of this disease. He even goes so far as to say that it is reasonable to conclude that the difference in the mortality between his own cases observed in Edinburgh and those of M. Louis in Paris 'was owing to the treatment, and that such is a legitimate application of statistics.' We may well ask what the numbers were which gave such remarkable evidence; they are 65 on one side of the Channel, and 75 on the other! In reply to this argument it is only necessary to cite the experience of other observers who have had a very much smaller mortality than Dr. Bennett; for surely he is in all fairness bound to admit that a man who only loses one patient in 60, or another who has actually only one death among 90 recorded cases, must have discovered a mode of treatment better than his own. Statements to this effect are made by a writer who has collected the largest number of statistics which I have met with on the subject; and it is very remarkable that though the cases just referred to are included among those in which venesection was not practised, yet the statisticians fail in showing the advantage of abstaining from bleeding in pneumonia. . . . The cases in which bloodletting formed one part of the treatment gave a death-rate of 164 in the thousand; while 10,000 cases treated almost entirely without venesection gave a death-rate of 203 in the thousand, not including the army statistics, which give only a death-rate of 39 per thousand during twenty years, when moderate bleeding was the rule of practice." (p. 49.)

Another therapeutical inquiry of the same Association is alluded to, in regard to the treatment of tape-worm by the oil of male fern. It is remarked that while no doubt can exist, upon inductive evidence, that this remedy will generally expel the parasite, statistics might have been made, with more advantage, to prove its *comparative* efficacy; whether it or kousso, for example, fails most or least frequently. Moreover, some light might thus have been thrown upon the causes of its failure in certain cases.

The treatment of non-syphilitic psoriasis, and that of scarlatina, as considered in other similar reports, are not regarded by our author as having been thereby greatly elucidated as yet; although the latter may gain somewhat of importance hereafter by the extension of the inquiry to a large series of cases.

The recent doctrine of "syphilization" is well contrasted by Dr. Barclay with that of vaccination; the latter affording a splendid example of successful induction, the former of futile hypothesis. As the preventive action of vaccination harmonized well with other known facts, and was itself suggested by observed instances, it needed but a few experiments to afford it a demonstrative support; while syphilitic inoculation as a conservative measure, finding no clear analogy, nor positive fact to father it, must die

almost stillborn, after a few failures, notwithstanding some promising coincidences. A similar fate, too, belongs to the idea of the use of belladonna as a prophylactic for scarlet fever, and of sarracenia as a cure for smallpox.

Acute rheumatism has been subjected to more experimentation, as well as hypothesis, than most maladies; though as yet without wholly satisfactory results. The same is remarked of pulmonary consumption. Dr. Churchill's method of treatment by the hypophosphites is "ruled out" by our author, we think very properly; valid evidence not having been afforded of its efficacy. For this, two series of cases should have been taken, as nearly alike as possible on all variable points; one set having employed the remedy under trial, and the other not. The number of deaths, in a term of years, in each series, and the number of recoveries, might give us some definite estimate of the value of the medication proposed.

Cholera has also been made the subject of many experiments, rational or irrational in their suggestion; often purely empirical. The investigations made by authority of the London Board of Health and Royal College of Physicians, as embodied in the report of Drs. Baly and Gull, appear to show that, urgent as are the claims of many for particular modes of treatment, "no induction has yet been established, indicating that any remedy possesses curative powers in cholera."

Dr. Barclay goes beyond his own rules of strict evidence in asserting without qualification (p. 94) that cholera is a disorder "derived more or less directly from another individual whose tissues have been similarly diseased." Further on in the volume he resumes the topic of the etiology of this disease, as illustrating inductive principles (p. 107). Dr. Baly's only generalization was that cholera is propagated by human intercourse, and that certain unknown causes aid or limit its transmission to particular persons. This our author calls "an empirical law." He adds that Dr. Snow's hypothesis in regard to the nearly universal transmission of the morbid element through drinking-water was essentially limited by facts brought to bear against it by Dr. Baly and others, showing that such is only *one* of the modifying influences affecting cholera. Dr. Barclay overlooks a larger generalization, first distinctly enunciated in this country,¹ which includes *all* of those influences, leaving as unknown only the specific morbid cause which gives to this epidemic its entity. This is, that *animal decomposition*, in any and every form, is the great *promotive* cause of cholera; acting only with the migratory specific cause, but determining, as to that, its direction and extent of propagation.

Dr. Todd's "supporting treatment," as applied by him, in theory at least, to all acute diseases, is well commented upon in the work before us. It is shown that a series of fallacies is woven through the whole argument on behalf of that theory and practice. Were it even granted that support is in all disease the most important indication, it does not follow at all that alcohol must be preferred for giving that support; nor is its use in such a mode, beyond what all practitioners have long agreed upon in certain cases, proved, by Dr. Todd's own statistics, to deserve confidence. As quoted by Dr. Barclay, of eighteen of his cases of rheumatic fever, fifteen are reported as having cardiac complication; and of continued fever, in twenty-four recorded cases, fully stimulated, eleven were fatal.

"In therapeutics," as our author says, "fallacies, from the misapplica-

¹ Philada. Medical Examiner, Aug. 1855; cited in British and Foreign Medico-Chirurgical Review, Jan. 1857, p. 62.

tion of the inductive method of reasoning, are very numerous." Perhaps there is something saddening in the view given in such a work of the great prevalence of medical errors. But it is a satisfaction at least to know that the profession is well awakened to the need, for its uses, of a positive science. The task now most pressing, for clinical observers, appears to be, to show that such positive science is possible; that facts in the action of remedies, in the modification of disease, are just as truly and substantially facts as those of chemistry, normal physiology, or vivisection; and that they may be relied upon, when well established, for that which we all yet hopefully labour for, however slow its progress—a really inductive science of medicine.

H. H.

ART. XXII.—*Medico-Chirurgical Transactions*. Published by the Royal Medical and Chirurgical Society of London. Second series. Volume the twenty-ninth. 8vo. pp. 447. London, 1864.

THIS volume contains twenty-four original communications. As abstracts of fourteen of these have been given in the numbers of this Journal for last year, we shall devote attention mainly to the other papers.

I. *A case of cancerous infiltration of the penis, with cancerous ulcer of the bladder, and secondary deposits in the lungs, bones, and other parts of the body.* By HOLMES COOTE, F.R.C.S., &c.

A short abstract of this case is published in the number of this Journal for January, 1864. It is a remarkable case, on account of the rarity of true infiltrated scirrhus elsewhere than in the mammary gland.

II. *Case of a mucous cyst on the laryngeal aspect of the epiglottis successfully treated by incision.* By ARTHUR E. DURHAM, F.R.C.S. Communicated by JOHN BIRKETT.

An abstract of this case is published in the number of this Journal for January, 1864. It may be added that the patient's difficulty of swallowing was first experienced immediately after a severe attack of sore throat, under which he had suffered two years previously to undergoing the operation by which he was so instantaneously relieved.

III. *Case of unusual difficulty in lithotomy arising from great distortion of the pelvis by rickets.* By HENRY THOMPSON, with a note by JOHN ERICHSEN, Esq.

This case is that of a boy, aged $4\frac{1}{2}$ years, from whose bladder, by the median operation of lithotomy, a stone was removed one and one-eighth inch in length, seven-eighths of an inch in breadth, and five-eighths of an inch in thickness. On the third day the patient died, after the usual symptoms of peritonitis.

At the post-mortem examination the upper outlet of the pelvis was seen to present an irregular heart-shaped figure, obliquely cordate; the sacral promontory approached within three-eighths of an inch of the left pubic ramus, within barely five-eighths of the right, and barely seven-eighths of the symphysis pubis. These measurements are those of the dried bones.

Mr. Erichsen's note relates the operation performed by him some ten weeks previously upon this patient, when finding that the calculus could not be extracted in the usual way by ordinary instruments, and that it was too